

to relegate the rare and occasional visitors—often of the utmost interest—to a future supplement, which will enhance the expense.

To the eye of an artist the plates will doubtless appeal as admirable specimens of the process employed, but to that of an ornithologist they lack the life and vigour which in many cases compensate for an absence of coloration.

Finally, we quite agree with Mr. Stonham that in many species the female and young are well worth depicting, and that it is quite useless to attempt to represent the songs of most birds by a set of syllables which each reader would in all probability mouth differently.

The Manufacture of Concrete Blocks, and their Use in Building Construction. By H. H. Rice and W. M. Torrance. Pp. 122. (London: Archibald Constable and Co., Ltd., 1906.) Price 8s. net.

THIS work is a reprint in full of the two prize papers on concrete block construction in connection with a competition instituted by the *Engineering News* and the *Cement Age*, and, in addition, abstracts are given of the papers of ten other competitors, which contain data not given in the prize papers.

Mr. Rice in his paper deals fully with the raw materials—cement, sand and gravel, or crushed stone; with the mixing and manufacture of the blocks; and with the important questions of curing and facing the blocks with a finer quality of the material, and he briefly discusses the principles underlying the use of this material in building construction.

Mr. Torrance deals more fully with the form of the blocks, illustrations being given of many of the moulds for which patents have been granted, and with the relative cost of buildings of concrete and other material; finally, he states that from an artistic standpoint the best success so far obtained has been where the process of casting in sand has been adopted, and several reproductions of photographs are given to illustrate this point.

The abstracts of the other ten papers give much useful information on many points of detail not dealt with by the authors of the two prize papers, with regard both to the manufacture of the blocks and also to their employment in building construction.

In an appendix are the rules and regulations governing the use of this material and the testing of the blocks in Philadelphia. There has been quite a flood of literature during the past year on reinforced concrete, but until this book appeared little had been written in reference to the use of concrete by itself for building purposes.

Elementary Electrical Calculations. By W. H. N. James and D. L. Sands. Pp. 216. (London: Longmans, Green and Co., 1905.) Price 3s. 6d. net.

THIS book is based upon a series of lectures given by the authors to first- and second-year students of electrical engineering, and can be confidently recommended to those for whom it is written. So far as it goes, it is well arranged and perfectly clear; the only criticism that can be suggested is that it does not go far enough. The range of a subject which should be studied by first- and second-year students is, however, a matter for individual teachers to settle.

It will suffice, therefore, to state that the book begins with an account of the fundamental units, proceeds to discuss Ohm's law very fully, and devotes brief chapters to power and work, conversion of energy, transmission and distribution treated quite simply, electrochemistry and photometry. Each chapter contains numerous examples fully worked out, and a large number of exercises for the student.

LETTERS TO THE EDITOR.

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of NATURE. No notice is taken of anonymous communications.]

Biometry and Biology: A Rejoinder.

I SHOULD like to preface my remarks on Mr. Lister's reply by relieving his mind from any anxiety about Dr. Pearl's feelings. Dr. Pearl is in America, and I cannot, of course, communicate with him, but I know him intimately, and am convinced that he is far too good a man of science to feel aggrieved by any criticism of his writings. He might well feel aggrieved that Mr. Lister supposes him desirous that his paper should remain un-criticised, because the criticism should affect his reputation. I am inclined to think that, as a fellow biometrician, he will rejoice with me that Mr. Lister's vague charge—made at a singularly unfitting moment—has been brought to a definite issue, and can be tried *coram judice*.

Had a first-year biometrical student in my laboratory sought advice from a biological freshman about the nature of *Paramaecium caudatum*, I should have anticipated that he would receive much the information with which Mr. Lister provides us. His remarks could only be made by one who (a) had either not studied the memoir he criticises, or had failed to perceive the significance of the constants calculated by the author, and (b) had never attempted accurate measurements on infusoria, or previously to such attempt been trained to that caution and accuracy in measurement which it is the function of biometry to inculcate.

I challenged Mr. Lister to substantiate the charge he made in August, when, presumably, the grounds of his insinuation at York were fresh in his mind. He then considered that Dr. Pearl's position was traversed by the objection that the conjugants individually are possibly or probably differentiated gametes.

What was the author's position? He expresses it exactly by quotations from Huxley and Romanes:—

"In my earliest criticisms of the 'Origin' I ventured to point out that its logical foundation was insecure so long as experiments in selective breeding had not produced varieties which were more or less infertile, and that insecurity remains up to the present time" (Huxley, "Life and Letters of Darwin," vol. i., p. 170).

"To state the case in the most general terms we may say that if the two basal principles are given in heredity and variability, the whole theory of organic evolution becomes neither more nor less than a theory of homogamy—that is a theory of the causes which lead to discriminate isolation, or the breeding of like with like to the exclusion of unlike" (Romanes, "Physiological Selection").

This problem of the divergence of individuals into varieties is the one selected by Dr. Pearl, and according to Mr. Lister is the best example by which he can illustrate his statement that biometricians do not select a sound biological problem "before bringing a formidable mathematical apparatus into action for its investigation." This is the "hare cooked before it was caught," to cite again Mr. Lister's phrase. Dr. Pearl shows that such homogamy exists in an extraordinarily high degree in *Paramaecium caudatum*. In other words, he has broken entirely novel ground, which, to say the least of it, renders Huxley's position no longer tenable. This is now admitted, albeit in a niggardly fashion, by Mr. Lister himself. In August he considered that Dr. Pearl's position was traversed by his omission to consider the differentiation of gametes which was possible or probable. He does not now even endeavour to show that it is traversed by this, but says that I have claimed for Dr. Pearl the first demonstration of the existence of this differentiation. In other words, he now admits that Dr. Pearl has fully considered the problem of differentiation. In fact, more than half Dr. Pearl's memoir is devoted to it. He further twits Dr. Pearl and myself with not distinguishing between a man and his gamete!

I turn to the last point first. I venture to think that the problem of homogamy is essentially the same in relation to both Metazoan and Protozoan, when we are considering its effect on the possible differentiation of species, and endeavouring to surmount Huxley's difficulty. Having shown myself that homogamy certainly does exist in one type of Metazoa, it was necessary that it should be shown to exist in the Protozoa, and for one type this is what Dr. Pearl has achieved.

Mr. Lister only obscures Dr. Pearl's statement as to the persistency of type in conjugant *Paramæcia*. Had he understood the constants dealt with by Dr. Pearl in this part of his paper he would have seen that they were what in biometry are termed *intra-racial* and not *inter-racial* values. The conclusions of Dr. Pearl have nothing to do with the inter-racial differentiation of gametes. Dr. Pearl grew *Paramæcia* under much variety of environment, and found that the non-conjugant type was highly correlated with the environment and the conjugant type singularly little affected by the environment. The whole inquiry was, of course, undertaken to illustrate Weismann's position, that while acquired characters are not inherited, the environment can influence inheritance where one cell is both soma and germ. In biology it has become almost axiomatic to assume that the Protozoa can inherit acquired characters on account of this identity, while in the Metazoa the acquired character of the soma is at the very least not usually inherited. Dr. Pearl brings out the all-important point that the gamete in *Paramæcium* is not, like the non-conjugant cell, markedly influenced by the environment. If Mr. Lister assumes that the characters acquired by the somatic cells are handed over to the gametic cells, this is, of course, to sweep away entirely the Weismannian hypothesis, and we may reasonably ask him for the quantitative proof of this assumption. The proof will at any rate go to the basis of the current hypothesis of "gametic purity." Mr. Lister asks for evidence of any relation between external characters in man and his gamete. The problem is not this, but the relation between the external characters in two phases of a cell which can be watched in passing from its conjugant to non-conjugant conditions, and that is an entirely different matter. Even here biometricians will shortly be prepared with an answer to the thus restated question,¹ although it has no relation whatever to Dr. Pearl's main point.

The remainder of Mr. Lister's letter would never have been written had he studied Dr. Pearl's paper or measured, as the latter has done, five or six thousand *Paramæcia*. The passage in Maupas was sufficiently familiar to me, and is actually referred to, together with the previous work of Hertwig, Gruber, and others on differentiation by Dr. Pearl himself in his paper. But the differentiation of two populations can only be demonstrated by an accurate quantitative investigation of the means, variabilities, and correlations of those populations. It may be rendered "possible or probable," as Mr. Lister held in August by a statement conveyed without detailed measurements in seven lines of print. In fact, Maupas says he has never found conjugants to exceed 225 μ , while Dr. Pearl, measuring immensely larger numbers, has found individuals up to 285 μ , a value considerably in excess of what was reached by the largest non-conjugant in his measurements, 275 μ . It will be clear in the face of such results that demonstration can only follow a study of large numbers and their proper statistical treatment.

Mr. Lister next proceeds to state that "every practical biologist knows" that specimens which have been preserved and fixed will be distorted. Will the reader credit the fact that pages of Dr. Pearl's memoir are devoted to a discussion of the methods needful to avoid distortion? Mr. Lister's statement only amounts to the confession that he himself cannot prepare undistorted specimens. The "practical biometrician" knows that the distortion can be almost entirely avoided by instantaneous killing of the *Paramæcia* and the avoidance of diffusion currents in

changing to the higher grades of alcohol. The method of attaining these results is amply discussed by Dr. Pearl, but their application depends, as in all such matters, on long practice, on training and on technique. It is open to Mr. Lister to assert that the methods adopted by Prof. Worcester and Dr. Pearl failed in their object; it is not open to him to insinuate that they have overlooked a distortion the danger of which is obvious to the merest tyro in biology. But as he has not seen the preparations he can only defend his assertion on the ground that the measurements show marked evidences of the irregularities which would be produced by such distortion; and here we see at once the absence of thorough examination of Dr. Pearl's paper by Mr. Lister. Only three of the fourteen series discussed by Dr. Pearl were from preserved specimens. The methods employed in the other cases were different, but the many series were not mixed, as might be inferred from Mr. Lister's statement. Even including Dr. Simpson's measurements, made in a wholly different manner, there is striking general agreement which is absolutely inconsistent with the amount of variation which would arise in the case of largely distorted forms. As Dr. Pearl himself says, "The good agreement is something . . . which probably no biologist would have predicted before the measurements were made. One has been accustomed to think of *Paramæcium* as a soft-bodied creature likely to show great and altogether irregular fluctuations." He then points out that *Paramæcium* is less variable than *Arcella*, *Eupagurus prideauxi*, or *Ophiocoma nigra*, all of which organisms have a more or less firm exo-skeleton. So much for the question of the influence of distortion.

Mr. Lister next proceeds to the assertion that *Paramæcium* being a "slipper-shaped" (!) animal, there would be difficulty in measuring the breadth of the non-conjugant as compared with the conjugant. He would never have made this statement had he read Dr. Pearl's paper, where the relative weight of length and breadth measurements is considered at great length. The statement is further mere hypothesis, and not the experience of one who has learnt to measure *Paramæcia*. The difficulty of measuring the breadth lies with the conjugating individuals and not with the non-conjugating individuals, for reasons amply set forth by Dr. Pearl. In the next place, Dr. Pearl's main argument is drawn, not from the breadth, but from the length measurements, and, lastly, had Mr. Lister followed the significance of biometric constants he would at once have seen that his hypothesis was invalid. If the measurement of the breadth were affected by a large source of error due to diversity of aspect when the *Paramæcium* is measured after death, there would be little or no organic correlation between length and breadth. Dr. Pearl shows that the actual correlation is markedly higher for the non-conjugants than for the conjugants, and is of an intensity which we might reasonably expect from previous investigations of similar organic correlations.

Lastly, Mr. Lister proceeds gravely to inform us of another source of error which he supposes to exist. He says that Dr. Pearl's non-conjugant population consists of heterogeneous material, in which the variability would be increased by the fact that it contained all stages of individuals in process of differentiation into gametes. It seems astonishing to have to state it, but as a matter of fact no less than six control series of *Paramæcia*, in which no conjugation at all took place, and which each numbered 500 individuals, are dealt with by Dr. Pearl in his paper and compared with the series of conjugant *Paramæcia*. The comparison between conjugants in a preparation and the nearest non-conjugants was made primarily to ascertain whether the cultures were in any state of local heterogeneity, so that a spurious correlation between conjugants would necessarily arise from their being drawn from the same part of the culture. It was precisely of the nature of the test used by Prof. Weldon and myself to ascertain if locality influenced the value found for assortative mating in man.

Further, the slightest examination of Dr. Pearl's diagrams would have shown Mr. Lister that the so-called non-conjugant population is widely separated in distribution

¹ I will venture on a prophecy, rash as it may seem, but based upon a general experience of cell division where the mother and daughter cells have not been measured under like phases, that the correlation will not be found to be less than 0.5 or more than 0.7.

from the conjugant, and only in the first days of the conjugating fit is there an approach to a very slight secondary mode at the conjugating type value. This may correspond in the first days to a very small percentage of individuals in a "conjugating mood" among the non-conjugants. The skewness, however, as measured by Dr. Pearl's numbers, although slight, is the *other* way, showing that the non-conjugant population might best be conceived as a distribution *wanting* a portion about the conjugant type, and not as a population with an addition on that side, as it must be in the case of a mixture of non-conjugants and potential conjugants. Taking the variability of (a) all populations in which there were no conjugants, (b) populations of non-conjugants in which conjugants appear, and (c) populations entirely consisting of conjugants, we have the three numbers 8.7, 8.6, and 7.9, which suffice to show that non-conjugants in a conjugating population are practically identical in variability with non-conjugants in a non-conjugating population, *i.e.* the potential conjugate is a very small proportion of the population, conjugation taking place rapidly after the conjugating phase is reached.

I am sorry to have to reply to Mr. Lister in this fashion. I fear, to use his own phrase, he will still "go away unedified" from "the biometric side of the church." But the time has come when vague insinuations based on no complete study of biometry must be replaced by some attempt to understand before criticism is passed. Above all, in a case like the present, a total disregard of the contents of Dr. Pearl's memoir and a suggestion that he has made errors and overlooked difficulties, which he has actually dealt with at every turn, is not to the credit of the critic. A man who has spent years in studying *Paramæcia*, and made thousands of measurements after much consideration of the difficulties, may reasonably expect a different type of criticism from another who clearly has attempted no such series of measurements, and whose authority for *ex cathedra* utterances may therefore be well called into question. Dr. Pearl's full paper is now in type, and I do not think his reputation will suffer when the paper is tested against the *a priori* criticisms which Mr. Lister has passed upon it.

KARL PEARSON.

Biometric Laboratory, University College, London,
October 12.

Radium and Geology.

IN NATURE of October 11 (p. 585) two letters appear on this subject, in reply to which a few words may perhaps usefully be said. Mr. Fisher's principal point is that if the earth's internal heat is maintained by radium, there is no room left for that shrinkage of the globe by cooling which some geological theories require. I think that the difficulty is only apparent. The duration of radium, it is generally agreed, is limited to a few thousand years. The supply must be in some way maintained, or there could be no radium on the earth now. Writers on radioactivity are generally agreed that the radium supply is kept up by the spontaneous change of uranium into radium.

Since radium is found in ordinary rocks, we must, on the received theory, suppose that uranium also exists in these rocks. It may be objected that uranium is never entered as one of the constituents found by chemical analysis. But, since the quantity to be expected is only of the order of 1/1000th of 1 per cent., this is not surprising. It might be possible, by very special methods, to detect uranium in granite, but I think in any case we may feel confident that it is there.

Everything depends on this initial supply of uranium. It gradually passes into radium, and, after that, into some inert form. The supply of uranium cannot last for ever. Its gradual diminution must involve the cooling and shrinkage of the globe.

It may perhaps be thought in these circumstances illegitimate to equate the escape of heat per second from the earth to the supply generated by radium in that time. There is reason, however, to feel pretty sure that thermal equilibrium is practically established in a time small in comparison with the duration of uranium, so that the rate of change in the amount of the latter can have no appreci-

able influence on the distribution of temperature in the globe at any moment.

Mr. Palmer suggests that if the earth's internal heat is due to radium, the moon ought to be internally hot too, and its volcanoes should be active. I discussed the question of the moon's internal heat in my first paper (Proc. Roy. Soc., A, vol. lxxvii., p. 472). I quote from that paper:—"It has generally been supposed that the lunar volcanoes are extinct. But that view seems to rest chiefly on an *a priori* conviction that the moon has no internal heat. As Prof. W. H. Pickering has pointed out, all those observers who have made a special study of the moon have believed in the reality of changes occurring there."

Even if there were good reason to be sure that the lunar volcanoes were extinct, that would still be inconclusive. For it is believed by many geologists that volcanic action is due to the penetration of surface water to the hot interior of the globe. Thus volcanic inertness may be due, not to the absence of internal heat, but to the absence of surface water.

R. J. STRUTT.

The Rusting of Iron.

IN my remarks on "The Rusting of Iron," published in NATURE of September 27, I directed attention to the fact that pure hydrogen peroxide solution was rapidly decomposed by cast-iron, the latter becoming covered with rust. This, I stated, "was, no doubt, due to catalytic action."

In his friendly criticism of my remarks, Dr. Gerald T. Moody writes, in NATURE of October 4, "that the metal becomes covered with rust in a few minutes, is not, however, to be referred to catalytic action, as Mr. Friend suggests, but is a consequence of the formation of acids by the oxidation of some of the impurities present in the iron, and of the subsequent electrolytic action."

That acids are formed in the above manner may be regarded as certain. These attack the iron, forming minute quantities of salts, which are decomposed by the oxygen of the peroxide, yielding rust, and liberating the acid, which can now attack more iron. In this way a small quantity of acid may be instrumental in oxidising a large quantity of iron. In other words, the acid is a catalyser, and the reaction is analogous to the rusting of pure iron in the presence of carbonic acid, oxygen, and water. The particular acid or acids which will cause this catalytic action must depend, of course, on the sample of iron used.

For the same reason "the intensity of action will be determined by the amount of acid formed on the surface of each particular sample of metal, when in contact with the peroxide." It is thus unnecessary to assume an electrolytic action, as Dr. Moody suggests. This is supported by the fact that the same result may be obtained by employing pure iron, and commercial hydrogen peroxide, which invariably contains hydrochloric acid and other impurities, as Dr. Moody has himself pointed out.

WÜRZBURG, October 9.

J. NEWTON FRIEND.

Optical Illusions.

IN your issue for September 27 a description of some optical illusions furnished by revolving fans recalled to my mind a very powerful illusion which I noticed some time ago, but for which I have not been able to furnish a satisfactory explanation.

A thaumatrope card (*i.e.* a card having a cage pictured on its one side and a bird on its other side) was mounted so as to turn round a vertical median axis at a speed of about two revolutions a second.

When an observer, viewing the rotating card from a distance of 5 feet or more, shuts one of his eyes, the card appears instantly to reverse its direction of rotation. (At the same time the axis of rotation appears to tilt a little away from the vertical.) On reopening the closed eye the illusion vanishes, and the card again appears to assume its true direction of rotation.

I showed the illusion to several friends, who all agreed as to its striking perfection.

DOUGLAS CARNEGIE.

Newcastle-on-Tyne, October 10.